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Comment

## ***Interactive comment on “Comparison of N. Atlantic heat storage estimates during the Argo period (1999–2010)” by N. C. Wells et al.***

**N. C. Wells et al.**

n.wells@noc.soton.ac.uk

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Referee 2

Major comments:

1) The manuscript contains a lot of redundancy and is heavily overburdened with figures and tables. A considerable part of them could be easily merged without any loss of information. Specifically, tables 1-5 must be deleted as the information given by these tables is better represented by the respective figures. Further, I do not see any sense in presenting the detrended time series (figs.9 and 10) in addition to the original C792 time series (figs.1-2).

Reply: Tables and figures have been reduced significantly and only retained where

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necessary.

2) According to the abstract one of the most important issues discussed in the manuscript is an impact of “a strong air-sea interaction event in 2009-2010”, which “reduces the upward trend 1999-2008”. However, no explanation is given in the manuscript about the nature of this event (only a literature link is provided).

Reply: More information has been added including 2 references in the results and discussion sections.

Moreover, inspecting carefully the HCA time series in figs 1-2 and figs.9-10 I was not able to identify any remarkable event which took place in 2009-2010. What can be seen is just a general slowing of the heat content growth starting at 2004-2005. In detrended timeseries this looks like a decrease in OHCA (figs.9-10). But, again, NO visible dramatic changes during the years 2009-10, so the explanation given in the manuscript seems to be wrong.

Reply: We have endeavoured to make these points more clearly in the text.

Figures 9-10 have been removed because they do not add to the points being made. The reason the signal is not very clear in these figures is that the heat content is for the whole domain 10-70N, and the cooling only occurs in the subtropical region. Higher latitudes have a warming throughout this period and therefore tend to work against the cooling.

Reply: These points have been added to the results and discussion sections of the text.

3) Another important issue (also noted in the abstract) is a significant difference between the two time series (TAMARA and EN3) in the layer 1000-2000m. It is not sufficient to make just a note on this difference, saying that further investigation is needed. That would suffice for a technical report. In the submitted paper a discussion must be provided on this important issue.

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Reply: This is a fair point but is beyond the scope of this study. We have put in place some discussion in the results section to cover this issue.

4) The EN3 and Tamara time series also demonstrate a quite different pattern for the uppermost 0-100m, with the Tamara time series showing a clear annual signal in contrary to the EN3 dataset, where no seasonality can be seen. This issue definitely requires an explanation.

Reply: We have added a new figure 1 where this small seasonality has been removed from data sets. We have mentioned this issue in the result section and referenced earlier papers. Ie Ivchenko 2010 where it has been discussed in detail.

5) The manuscript is badly structured. For instance, a detailed description of the deficiencies of the ARGO dataset is given in the introduction (lines 20ff page 2364, till line 10 page 2365). This piece of text should be placed into the method and data section. Sections 3.2-3.5 give another example of the bad manuscript structure, with sections being too short and the description jumping from one issue to the other.

Reply: We have endeavoured to produce a more coherent structure to the paper.

6) The usage of different climatologies is obviously an important issue and should be analyzed in more details. I would prefer usage of one and the same climatology for the both experiments.

Reply: The paper rather mis-represented the EN3 data. It in fact uses WOA 2001 ( the same as TAMARA) as well as other data sets. . This information is added to the paper in the methods section and discussed in some detail. To complete an analysis of the different climatologies is a major task which should be done, but is beyond the scope of this paper.

7) The description of the OHC calculation method is unsatisfactory (lines 18ff, page 2367).

Reply: We have added a clearer explanation in the methods section.

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8) Other comments: Page 2364, line 5 : “other platforms”. XBTs and CTDs are not “platforms” - they are instruments, which can be implemented from different kind of platforms (e.g. ship, helicopter).

Reply: We have altered this in the abstract and in the main body of the text.

9) Page 2365, l26ff: why it is difficult to validate the argo data set? I can not see any connection here to the uniqueness of the argo dataset.

Reply: The description of the Argo data set has been revised and this part has been removed.

10) Page 2366, line 1: “..dataset employs a wide range of data” - bad wording. The dataset contains data from different instruments.

Reply: Agreed and this has been clarified.

11) Page 2370: “to further investigate..., the question arises..” The sentence must be completely reformulated. Section 3.2: as noted above, the section must be completely re-written as no signs of that “cooling event” can be seen on time series.

Reply: We have fully revised this section and clarified our consideration of the cooling event.

Page 2373: Line 1: “To further investigate ..., the question arises “ - bad wording!

12) Page 2373, Line 17: “it is been found” change to it has been found”

Reply: The discussion section has been completely revised.

The discussion and conclusion section is too short, no sufficient explanation of causes leading to differences between the two time series was given.

Reply: We have revised the discussion section and have also added some discussion in other parts of the text where it was necessary. We have mentioned possible causes and given extra references, but to definitively attribute the results to particular mecha-

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nisms would require a more detailed study with the use of a high resolution model.

Interactive comment on Ocean Sci. Discuss., 10, 2363, 2013.

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**OSD**

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